SCIENCE

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NEW YORK, SEPTEMBER 9, 1892.

THE IMPURITY OF ICE.

BY CHARLES PLATT.

WHEN we consider the large quantities of ice used annually in the United States, especially during the summer months, when drinking-water without ice is indeed a rarity, it seems strange that the purity of the ice should so seldom be questioned. On the contrary, while many people will exercise the greatest caution regarding their drinking-water, the origin or source of their ice-supply troubles them not at all. The ice-man brings it, and there is no further need of investigation. This feeling of danger on the one hand, and perfect security on the other, have been, without doubt, due to that peculiar science, "popular chemistry," so widely disseminated in various publications of the land, and especially in the newspapers of the smaller towns with their "patent insides" bristling with various startling items, often of scientific import.

That this theory, once so popular, of the purification of water by freezing is not in accordance with facts is hardly worth demonstration; we will simply state that while water in its crystalline state should theoretically be nearly pure, still, owing to its peculiar formation in needle-like crystals, considerable foreign matter present in the water in suspension may be, and is, mechanically held within the mass.

On the other hand, of late years there has been a revulsion of public sentiment, and statements have been made denying that water is purified at all by its crystallization. Others have gone so far as to say that certain bacteria thrive best at this low temperature, and, whilst they may be harmlessly disseminated through the water, in the ice they have become concentrated and doubly active. This certainly cannot be sustained by any known facts regarding bacteriological growth, and yet may have some foundation in the fact that in the freezing of still water, such as that of a pond, or small lake, there is a certain concentration of some species of bacteria at the surface of the water, and thus the first inch of ice may contain these in increased numbers as compared with a sample of water from the same lake. As to the increased activity, or indeed as to any vital activity at all of these bacteria, this is another question, and yet to be proven. When this ice is melted, however, and the temperature of the resultant water is raised maybe to the full heat of a summer's day or to that of an over-heated room, then we have another problem, that of possible decomposition and organic change in these organisms that may induce results equal to and exceeding those of the bacteria themselves.

We do not mean to say that all bacteria are necessarily killed outright by the freezing of the water, — the contrary has been proven in many cases, and notably by Dr. Prudden in his well-known experiments with the germs of typhoid fever, — but, in this as in other cases, the germs were found to be present always in decreasing numbers, and in longer or shorter time their vitality is lost altogether. Disease has undoubtedly been produced by use of ice from polluted sources, and this, too, when mere analysis of the ice in comparison with water standards would by no means condemn it. But here let us state that we can by no means use water standards in the analysis of ice. The standards must be much higher, and the analysis must needs be conducted with great care in order to arrive at correct results, even in cases where the ice may be in a manner contaminated.

In regard to the preparation of samples for analysis, Wm. R. Nichols, in his admirable work on "Water Supply," gives a few simple directions that every chemist will do well to follow. "In melting ice for analysis, a fair specimen cake should be selected and broken into fragments in a clean place. The fragments may then be placed in a wide-mouthed bottle covered with a plate of glass, and when enough of the ice has melted to have washed itself, this portion of the water is poured away and the remainder, after melting, subjected to analysis." The same author points out the danger of exposing the melting ice to the air, owing to its liability to attract organisms from the air itself, thus vitiating entirely the results of the analysis.

In the year 1888, the State Board of Health of Massachusetts was directed "to make a special investigation with reference to the pollution of ponds, lakes, streams, and other bodies of water used as ice supplies" in that State. The results of this investigation, which was admirably conducted, are of extreme value; as, by the systematic methods adopted, they were enabled to secure considerable data of general and vital interest to the public as well as to the chemist. Fifty-eight sources of supply were examined, their chemist making, in all, analyses of seventy-six samples of water and two hundred and thirty-six samples of ice. The deductions drawn from their work, stated as concisely as possible, are as follows: In the formation of the ice the color and saline matter of the water is completely removed, and also all but 13 per cent of the other impurities. The same cake of ice will vary greatly as to quality in different parts. The substances in solution are excluded from the ice in much larger proportion than are those in suspension. Of the different kinds of ice, classified as snow ice, bubbly ice, and clear ice, the two former contain a far greater percentage of impurity than the latter, while the upper third of the cake in all cases carries a higher percentage of impurity than the lower two-thirds.

Snow falling upon thin ice will cause it to sink, and finally the snow will become saturated with water; which, in turn freezing, includes within the mass all of its impurities. Then, too, the falling snow has already collected from the air such impurities as were contained, and those also are to be found in the resultant snow ice. The flooding of an ice-field by cutting holes and allowing the water to spread over the ice surface, there to freeze, gives a layer of ice of equal impurity with the water itself; and yet this method of rapidly building up the ice-crop is practised to a great extent throughout the country.

That the bacteria are largely to be found in the surface and snow ice can be seen by the following figures, arrived at by the above mentioned Board of Health. In snow ice was found 81 per cent as much bacteria as in the water itself. The ice as a whole contained but 10 per cent, and the clear ice but 2 per cent. The average results of all the analyses made by the Board of Health show that the total organic impurities of snow ice amount to 69 per cent of those of the water, and the organic impurities of all the ice except snow ice 12 per cent, while the clear ice contains but 6 per cent. The color was entirely removed and the saline matter nearly so.

As mentioned above, the impurity of the snow ice is not due to the water alone, but the air as well must be looked to to account partly for this great increase of organic matter.

These figures might tend to give one a feeling of great security in using ice, as at first thought the decrease of organic matter in clear ice from that originally present in the water itself would seem to argue perfect safety in using ice from any ordinary supply. The Massachusetts Board of Health, however, carefully point out that it is not the number of bacteria alone that is to be considered, but their kind, and they insist that no water-supply that is not fit for drinking purposes should for any reason be used as a supply for ice; and yet how often is this the case; stagnant ponds and even sluggish canals receiving their drainage from

many sources being all thought fit to yield their share of the ice harvest.

Fortunately of late years, owing to the repeated failure of the ice-crop, the larger cities in the east are mainly supplied with "artificial" ice. This ice, being formed as it is in the greater number of cases from the regular water-supply of the city, ceases to a large degree to be a source of danger from organic contamination. There have been cases, however, and notably one which fell under my own observation, where an ice company, advertising their ice as made only from pure distilled water, produced daily for some weeks beautiful cakes of crystalline ice, the centre of each cake a rich, dark-brown, and actually giving forth an offensive odor! Some of these samples were sent to me for analysis, and the results were most startling, indicating rather a concentration of impurity, both organic and inorganic, than a distillation or purification. The cause was naturally looked for and found in the stills themselves, which were eventually overhauled and remodeled, with the result that finally a first-class high-grade ice was put on the market.

The necessity for an absolutely wholesome water-supply for the manufacture of ice is at once apparent, as in processes generally in use the entire contents of the water tanks are frozen, and all impurities contained in the water must needs enter the ice. The case referred to was interesting, showing as it did how the color and organic matter had been concentrated in the middle of each cake. The ice forming first at the sides had repelled these impurities until finally, with the freezing of the entire mass, they had of necessity been included.

ELEMENTARY SCIENCE IN THE PUBLIC SCHOOLS.

BY HENRY MONTGOMERY, PROFESSOR OF MINERALOGY AND GEOLOGY IN THE UNIVERSITY OF UTAH, SALT LAKE CITY.

MANY years' continuous service as a teacher of young men and women, in a measure, unfits one for acting as an instructor of children. I do not say that a teacher of children requires greater or higher qualifications than a teacher of college students; but the qualifications must be different. One who aspires to be a teacher and leader of students of advanced subjects as taught in colleges and universities ought to have good mental faculties, and these ought to be in a high state of cultivation. With the increasing intricacy and complexity of the studies come increasing difficulties for the students. These difficulties must be recognized and dealt with by the instructor. Hence the successful university teacher must be possessed of teaching powers suited to the minds of students of the advanced branches in which he undertakes instruction. Both the character of the studies and the mental condition of the students of the university differ widely from those of the pupils of the common school. Consequently the teachers of these two classes of pupils must differ widely as to qualifications. Between the primary or common school, on the one hand, and the university, on the other, comes the secondary or high school, which, of necessity, must be supplied with teachers of somewhat different qualifications. The high-school teachers must be adapted to the work of instructing pupils of certain attainments and generally of a certain average age, which stand between the common school and the university. A still more satisfactory grading is effected by classifying all pupils in five divisions, viz., the kindergarten, the common school, the high school, the college, and the university; and in these, especially in the common school, a further grading often proves beneficial. It has many times been found that one who has succeeded well teaching a class in some branch in the common school, has not succeeded as a teacher of a lower class or a higher class in the same branch and in the same school. We all know that a child will voluntarily leave other children that may be older or younger than himself, and seek out those of his own age, or, rather, of his own mental attainments; and, again, on reading a story to a child of nine years no interest is awakened, while on reading the same to another two or three years older or younger the most lively interest and appreciation of it are immediately manifested. The first thing, then, to be considered in the teaching of science is the stage of the development of the faculties of the child. Let this be first diagnosed, and then let no mistake be made in prescribing the kind of material suited to his condition, and the character of the methods of instruction to be employed in his particular case.

To the question, Should science be taught in the public or common schools? I answer in the affirmative. Most decidedly, yes. Which of the sciences? Should it be chemistry, or physics, or zoölogy, or mineralogy, or botany, or physiology, or geology? I answer, all of them as one subject, the study of nature. Specialization, differentiation, or the division of labor, characterizes civilization. It is forced upon us in the higher studies. This is simply a matter of necessity, due to the vastness of the fields of higher learning, the shortness of life, and the limits of the human mind. But, it is possible to specialize only in the maturity of manhood and womanhood. It is not possible in childhood. The youthful mind is not capable of such work. The young mind is not able to fix attention or concentrate thought upon a subject, and particularly if the subject be studied in an isolated and disconnected manner. Add to this a method that is both systematic and abstract and the avenues to learning are completely closed. In very early years, say before the age of eleven or twelve, the average child cannot readily or profitably study anything in an isolated, a systematic, and an abstract manner, and he can do it but very feebly at this age. The study of a subject systematically by classification, the study of the abstract, and the cultivation of the reasoning faculty should not be attempted early. Nature rebels against it. It is the faculty of perception which appears first. This is the faculty which should receive the attention of the teacher of children. To the cultivation of observation, expression, and memory, along with the full physical development of the child, all the best energies of the teacher should be given. It is not a question, then, of dividing and classifying the natural and physical sciences, and choosing one or more of them to be placed on the curriculum of schools. This is necessary and proper in the later years of the high-school courses, and in the higher institutions, but not in the common school, or to any great extent in the lower classes of the high school. System, method, and classification in study are exceedingly important for matured persons; but, they do not belong to early life. As soon as the mind is prepared to undertake such work, it should be begun; and it should be increased very slowly, gradually, and almost imperceptibly. I repeat it, common-school pupils ought not to be taught zoölogy as a distinct science, nor botany, nor physics, nor geology as such. All systems of classification, even to the division of these sciences, are artificial. Chemistry, physics, mineralogy, botany, zoölogy, physiology, and geology should not be separated. These sciences come naturally together; and, therefore, they are most readily understood and remembered when studied in this way. Let the child see the fish swim in the water, the bird fly through the air, the duck swim and sail on the pond, the river erode its banks, the waves beat and grind the pebbles against one another on the beach. Let him be led to use his senses in observing the soil, clay, sand, gravel, grasses, trees, flowers, butterflies, beetles, worms, crops, streams, hills, ravines, bees, squirrels, ants, crickets, birds, snow, rain, stones, rocks, and fossils, just as they occur in nature. In any case, even to adult persons, the associations are of vital significance. Many a time it happens that a mineral sample, a bit of rock, or a fossil, by itself is of but little use in helping us to understand some question of moment. Again, an extract from a book may be unintelligible or ambiguous. But, in the one instance, permit us to see the associated minerals and rocks in position, and, in the other, to read the context, and what a flood of light is let in upon us! The relations that objects of the three kingdoms of nature bear towards one another are of the utmost importance. But, in addition to the importance of the associations and relations, the ease with which children are enabled to comprehend the characteristic structure, habits, and uses of anything when studied as it occurs in nature, is something the teacher and parent cannot afford to ignore. An old-fashioned method of teaching orthography consisted in compelling the pupil to learn a column or a page of isolated words chosen with reference to the number of syllables they contained. Some of these words were extremely rare; many of them would not be

used by the pupil in speaking or writing, or met with in reading, for several years afterwards, and all of them were completely separated from the other words requisite to constitute any sentence. This was an unnatural method, and, it is needless to say, an unscientific method. By exercises in composition, and also by means of dictation exercises, i. e., a careful selection from an interesting story or history, suited to the capacity of the pupils, and with just enough new words and idioms in it to ensure progress, orthography is taught scientifically. By this means the child sees the relations of the words, understands their uses, and so more easily remembers and uses them. Thus it is in the teaching of science to children. Is it not true that natural and physical science is even now taught in many of the schools of the United States in very much the same manner as spelling was wont to be taught in olden times, in a disconnected, detailed, and unnatural way? But, it may be objected that the curriculum is already loaded with studies, and, therefore, there is not room for all the sciences on the common-school programme. My reply is, that in place of increasing the load I would actually lighten it. Requiring elementary, practical, concrete, object-instruction in nature does not imply an increased amount of work. The sum total does not need to be greater. If demanded in the interest of health, the total amount should be lessened. But the work should be natural; and, being more natural, it will, of course, be lighter and more acceptable. The study of nature is pre-eminently that which cultivates observancy, and accordingly comes first. Yet, it cannot be taught without a language; and the language in this country must be the English. Writing, spelling, reading, grammar, composition, drawing, geography, and arithmetic can all be taught while giving instruction in the natural and physical sciences. In fact, the teaching of science to children implies practice in drawing, writing, oral composition, written composition, and a certain amount of arithmetic. An afternoon's, or, better still, a forenoon's ramble over the fields, up a canyon, upon the side of a mountain, or along the shore of a lake or bank of a river, or a visit to a good museum, will ordinarily afford abundance of material and opportunity for penmanship, letter-writing, drawing, measurement, calculation, and oral and written language lessons.

Hence, it is plain that it is not a specialist in any particular branch of the sciences who is needed to teach children. The teachers should be chosen with reference to their fitness for teaching children of a certain stage of mental development. This is the natural standard. It is not really necessary that the teacher have a college education, or a knowledge of the advanced studies. But, it is absolutely necessary that the teacher be possessed of good common-sense, be able to see clearly the things around him, be accurate as far as his work extends, and be full of love for that work. There is a little book entitled "Directions for Teaching Geology," by Dr. N. S. Shaler, professor of geology in Harvard University, which ought to be in the hands of every common-school teacher. After an experience of twenty years, teaching all grades of students, Dr. Shaler expressed himself as follows: "It seems to me very desirable that the first steps of the child in the study of the physical world should be given by teachers who give the beginnings of the other branches of learning. Although it is held by some students of the problem of science-teaching that the work must be done by special teachers of science, I am inclined to believe that the view is a mistaken one. The special teacher will have to divide the intellectual life of the student, and in the infantile stages of this education it is difficult to make this division."

As to methods of instruction in elementary science, the judicious use of books, pictures, charts, maps, and models is proper. But the instruction should be largely by open-air excursions of two or three hours each, and taken twice or thrice a week. It can easily be accomplished in most places throughout the greater portion of the year. Of course, in inclement weather the instruction must be given in the schoolroom. It is there the material collected by the teacher and pupils in their rambles may be examined and studied. The schoolroom may serve the good purposes of a shelter, and a place for exercises in description of the excursions and of the specimens gathered. It should also be used for the inspection and study of manufactured articles, which are, of course, the products of scientific industry. These exercises should be varied. Oral questioning is useful. Written descriptions are still more useful. Drawing is of great service as an exact form of expression; but much care should be taken to prevent it from becoming merely a mechanical exercise and thus interfering with true inspection. This indoor work is especially useful for reviews. In reviews, the memory becomes trained and strengthened; and let it not be forgotten that the memory ought to be trained and strengthened. Memory is an important faculty. I have no sympathy with the modern tendency to despise memorizing. I believe strongly in the cultivation of the memory. What would man be without a memory? As a matter of fact, I know of nothing that hinders and cramps my teaching more than the lack of a strong, full, retentive memory on the part of the student. Often it is with difficulty he can recall the meaning of words in his text-books. He cannot follow me because he has forgotten the significance of many of the words, English as well as technical, used in the lectures or explanations. For the same reason, many times he omits taking notes, or else he causes delay by stopping the instruction in order that whole sentences may be repeated for his benefit, his memory not being strong and active enough to grasp and retain more than a very few words at any one time. Why should so many young people enter college at the age of sixteen or seventeen with weak and leaky memories? If the question had reference to the reasoning faculty, it could be satisfactorily answered, inasmuch as reason appears later than memory, and has not had time for development. But, the memory can and should be developed in the primary and secondary schools, and the study of nature is eminently adapted to this development, as it is also to that of comparison. I am disposed to think the high schools might do much more in this direction by wisely conducted examinations upon large portions of the work. I say "wisely conducted;" for I know there are examinations that are not wisely conducted, and such would not produce the desired result. It is not enough for a young man to tell me that he knew a subject one or two years ago. If I wish to engage him to do a certain kind of work in which a knowledge of that subject is required, I wish to ascertain what he knows about it now, and whether he can use it now. Discipline of the mind is one thing, and practical knowledge is another. True education must include both. A student may shine in the class-room day by day; yet he may not be able to pass a good examination or even a fair examination on the whole year's work. He does not possess that particular kind of power which will enable him to hold a year's work. This faculty should be recognized and improved.

With reference to the mode of instruction by frequent excursions, that the young people may, under a competent guide, get a first-hand knowledge of nature for themselves, allow me to call attention to an article from the pen of Dr. J. M. Rice, which appeared in a recent number of the *Forum*. In the article referred to Dr. Rice sketches the work he saw done in primary schools in Germany and New York during his visits to both. He commends the German method by field-excursions as being scientific, and condemns the American method, or that which he witnessed in New York City, as being unscientific. The contrast between the two systems is very forcibly brought out. Dr. Rice concludes his interesting article in these words: "If life be made a burden instead of a pleasure to the child, the blame falls upon those persons who fail to place their children in the hands of individuals who know how to educate them without destroying their happiness." This I take to be an appeal from Dr. Rice, not to a few persons, but to the American people at large, to free their children from the evils of a close-confining, hot-house, mechanical system of primary education. Doubtless in many instances the teachers are to be pitied rather than blamed. Assuming them to be qualified and reliable teachers, desirous of taking the little ones out for study, the principal, if there be one, or the superintendent, may possibly object; perhaps some members of the board may object, or the children's parents may offer opposition. Under these circumstances, what are the teachers to do? Simply stay shut up in the school-house during the finest weather, and

be obliged, four or six hours a day, to teach, as best they can, perhaps without maps, globes, charts, models, pictures, or any other appliances of a proper sort. This would be somewhat bearable, aye, even profitable, were the pupils sixteen to twenty years old. But it is a terrible thing for children, and a terrible thing for their teacher, to be expected to endure. Such teachers and children have my sympathy. I sincerely pity them. Think of trying to hold in quietness and attention in a schoolroom, for hours at a time, forty or fifty children, whose tender, growing bodies and minds call loudly for air, for sunshine, for exercise and freedom! What is the use of talking about teaching science so long as in our very attempts to teach it we continue to act in opposition to the laws of nature? A striking example of this inconsistency occurred some years ago in a well-known eastern university. While the professor was lecturing on hygiene, one of his students fainted for want of pure air, the room being closed up and utterly destitute of ventilation. With the view of enlarging and improving the facilities for elementary science instruction, permanent collections might be made in every school. But really good collections, kept in proper order, cost a great deal, and, consequently, must be few in number. Good museums in cities might be made highly useful to all the common-school children within easy reach of them. They would be a relief to all in winter, and they would at all times be useful to those whose school buildings might chance to be situated near the centre of a large city and at an inconvenient distance from the fields and hills of the open country. Especially should we expect the State university museum to be made convenient, attractive, and instructive to all grades of young and old students. All parts of our public educational system should be consistent and in harmony. If a university can be equipped in such a manner that visits to its museum and inspections of its collections may be a source of pleasure and instruction to the pupils of the public schools of the State, or even any considerable part of the State, it will surely be so much the better. Well-arranged, well-labelled, and well-lighted university museums may and should serve as great educators of common-school pupils, as well as of the general public, who may visit them from time to time. In California, and at least one or two other States, the express companies carry specimens for the State universities free of charge. In New York State and some other places, the students in training at the State normal schools are required to pay only half the railroad fare to and from these schools, although many of them travel several hundred miles to reach them. A number of the eastern and northern States have for some time been furnishing free text-books for the public schools; and, in Ohio, not only are free text-books provided for the school children, but the State legislature has also taken measures for the supply of clothing for the pupils where it may be necessary. Now, as before stated, museums of any great value or importance must be few. They are too expensive to be numerous in an ordinary State. They cannot be transported from town to town. Why should not some arrangement be effected by which pupils of school age, and in regular attendance and full standing in the schools, and their teachers might receive free railway transportation at least once a year to and from the State university museum? Knowing what is done for public schools by a few large museums, I am of opinion that greater efforts should be made in this direction in all the States, and also that strong efforts should be made to better the university collections, keeping in mind the necessities of the public schools.

Were I asked for other advice regarding methods of teaching elementary science, I would say, that the pupils should be started with the study of the familiar, that which is most readily observed and best known. The subject matter should consist of common things, and the language of the teacher should be simple and intelligible to the pupil. Some twelve or fifteen years ago the distinguished scientist, Professor Huxley, published a book on practical biology, in which he adopted and advocated the system of study by which the student begins with the lowest and simplest forms of life and proceeds to the higher and more complex organisms. Owing to the fact that the lowest living beings are microscopic and obscure, this was altogether unnatural and unscientific. Yet, because Huxley adopted it, almost every teacher of biology, in English-speaking countries, adopted it too. Within a few years it became evident that (except with advanced and well-trained students) the results were far from satisfactory; and, accordingly, in the preface of a later edition of the book, Professor Huxley writes that experience has shown that the order ought to be reversed, and that henceforth the student should begin with those forms of life which are somewhat familiar, and proceed to those less known.

In the next place, I would warn the primary teacher against teaching the details of any subject to very young children. Unwise choice of material, and the forcing of a heap of details upon children, correspond closely to the old system of teaching spelling by selecting long and very rare words. This far-fetched material should never be used in primary teaching. Only the more conspicuous and general characters, uses, etc., should be dwelt upon, unless, in very exceptional cases, where, for some good reason, the child may appear to be profited by a minute account of any animal, plant, or mineral. In all cases, the details are most out of place when there is no object of the kind present. As far as possible, the teacher should keep close to the wishes and inclinations of the child in the choice of subject matter, and work along these lines, so long as there seems no good objection to his wishes. With high-school pupils, I would recommend the frequent use of the microscope. In the hands of an intelligent teacher, this instrument may be used to advantage with small classes of pupils, say, above thirteen years of age. A stereopticon or projecting lantern should often be used in all grades of schools. Certainly for a high school, no better investment can be made, and the common schools of any city might, by arrangement with the high-school teacher, who operated the lantern, become recipients of the benefits to be derived from the possession of this piece of apparatus.

A word or two with regard to physiology and hygiene. I consider that the teaching in these subjects should be greatly improved. I would not have a great amount taught; but, in several respects, it needs to be made more practical. Time will not allow me to expatiate upon these matters here. Yet I cannot refrain from directing attention to the fact that, for reasons of delicacy, three systems of organs of the human body, either partially or entirely, are invariably omitted from the course of instruction in all of our schools. For both moral and sanitary reasons, I am inclined to think something should be done, and that something will yet be done to provide for a wholesome, intelligent, and practical course in these subjects. It may be that at present little can be done; but I venture to suggest that where it is altogether practicable to do so, perhaps in some city high schools, the sexes receive instruction in these studies in separate class-rooms, and from capable and proper instructors. It would, of course, be absolutely essential that the instructors have properly constituted minds, and be especially qualified to speak to and deal with young persons, in order that good might come of their instruction. This is undoubtedly a difficult problem to solve. It must, however, be admitted that it is a very important one.

Again, the science teacher must have interest in the studies themselves. I have not much faith in the common, little, artificial devices for exciting the interest of the pupil. They are but the nostrums of quack doctors. They remind one of the application of ointment or salve to the external surface of the body to cure a disease which has its seat in impure blood or in a weak nervous system. They are not born of sympathy or interest in the study. The teacher should be interested in the studies as well as in the pupils. It is all right to desire to do good to the children, but there must in addition be a pleasurable enjoyment felt by the teacher in the prosecution of the study itself. In fact, interest in the study — a spirit of inquiry, of enthusiasm, if you will — is of the utmost importance. Teachers and pupils alike need it. Teaching must not be done merely for money; it must not be done in order to show one's knowledge. The pupil must not ask questions with the view of puzzling the teacher, or of showing his own learning or smartness. Too often do we get students who have been so praised and flattered by their previous teachers, that it is exceedingly difficult and sometimes impossible ever to do

anything for them. They are keenly disappointed at not being acknowledged as perfect in their studies, and are ever anxious to show their learning. It takes a long time to work them down to their proper level. Teacher and pupil should ever seek truth. They must come to their work in a spirit of earnestness, absolute honesty, candor, and sincerity, otherwise the work will be a failure. The really true teacher is an inspired man. He draws the pupils around him, because he is himself interested in his studies. Such were the great teachers of old, and if any of us now succeed in any measure as teachers, it is only so far as we possess interest and enthusiasm in our studies.

Frederic Harrison, with forty years' active experience in educational work, in writing of late, said: "I have for years past joined in the discussions and conferences on this question; and now I feel at times that we are further off the right path than ever, as if our whole system were a failure. There are hours when I feel about education nothing but this, — wipe it out, and let us begin it all afresh." This was written a few months ago with reference to education in England; but it was in relation to some of the very matters that are engaging our attention in this country at the present time. I cannot go so far as Harrison does in this expression of his opinion. I know the public schools of this country have done and are doing a useful and a noble work. The nation cannot do without them, nor can it afford to permit their usefulness to be impaired through lack of support and sympathy. Give them the support and encouragement they need and deserve, and they will be improved, and the country profited thereby. Honest and intelligent interest in the schools should lead to improvements in their condition. If changes in the system seem desirable, let them be made. Let neither prejudice nor individual selfishness stand in the way. It has more than once been stated by American educators of experience and high standing that science-teaching is difficult, and that there are few, very few, teachers capable of engaging in it. I fear there is much truth in this statement. Science, like any other subject of education, must be taught by a competent person. It is folly to expect proper results from persons who have not both the natural and the acquired qualifications of a true teacher, and it is much greater folly to expect them from those who have neither of these two qualifications. Teachers possessed of both are indeed rare; and how can we expect them to be plentiful so long as the trustees and boards of education, and the people behind the trustees and boards, remain satisfied with so low a standard? When the public come to realize that a higher standard of qualifications, mental and moral, on the part of the teacher, is absolutely necessary for the welfare of our country, when they come to have a heartier appreciation of high-class attainments, they will be willing to make adequate compensation for the teacher's labors and influence, they will seek teachers of longer and better training and experience, teachers who carry with them an atmosphere of a higher and a more inspiring character. I have hope that this time will come. Let us do what we can to bring about these conditions. For the sake of the youth of our land, for the sake of the material, the physical, the moral, and the intellectual advancement of our country, for the sake of everything that can contribute towards the promotion of the civilization of this great nation, let us earnestly pray that the dawn of that day may be hastened, that the free public-school system, which forms a net-work throughout the length and breadth of this Union, may, more truly and fully than ever in the past, yield those practical and beneficent results anticipated by its founders, hoped for by its friends, and rendered necessary by the foundation principles of the government of a free people.

NOTES AND NEWS.

A REPORT on the petroleum trade of the Caucasus has been sent to the Turkish Government by Aassib, the Turkish Consul-General at Tiflis, and some interesting extracts from it are quoted in the *British Board of Trade Journal*. The petroleum springs of the peninsula of Apcheron, not far from the place at present occupied by the town of Baku, were known, according to the writer, several centuries before the Christian era, and the phenomena produced by them, totally inexplicable in those barbaric ages, gave rise, he says, to the worship of the Guebres, followers of Zoroaster, which lasted into the nineteenth century, for the temple of the worshippers of eternal fire is seen to the present day. The springs of Balakhani are situated 20 kilometers from Baku on a bare and arid plateau, swept by the winds, at an elevation of about 60 meters above the level of the Caspian Sea. The petroleum lands occupy an area of about 8 kilometers. At the present time Balakhani and Sabountchi possess more than 1,000 wells, some of them newly bored, producing in twenty-four hours as much as 400,000 pouds. An era was marked in the history of the naphtha industry by the house of M. Nobel, which started at Baku in 1874, and in the following year purchased a small business and undertook the production of petroleum on a small scale. At that time the conveyance of petroleum to Baku was effected by means of carts and leather bottles. M. Nobel endeavored to show the absurdity of this primitive method of transport, and recommended that pipes should be constructed, but the majority of the merchants rejected the proposal. He then constructed the first pipe at his own cost, and demonstrated the utility of it to his colleagues, several of whom very soon imitated his example, and Baku has to-day a dozen lines of pipes, each of which cost more than 100,000 roubles. The same house, dissatisfied with the system of shipping petroleum in barrels, proposed to the Kavkaz and Mercury Navigation Company of the Caspian and the Volga that they should build tank-boats for the exclusive conveyance of petroleum. This proposal having been rejected, the firm constructed several of these vessels at their own expense. This innovation, of which even the Americans had not yet thought, was accepted by the two petroleum-producing countries, and tank-boats, the number of which is constantly increasing, are to be found on all the waters of the civilized world. It is also to M. Nobel that those gigantic reservoirs of iron which contain hundreds of thousands of naptha products are due. They are to be seen in large numbers at Baku, Batoum, and everywhere else where petroleum is carried in bulk. The series of innovations by M. Nobel do not stop there. With a desire to improve land-carriage he proposed to the Griazi-Tsaritsine Railway Company the construction of special tank-wagons for the transport of the petroleum, guaranteeing a load for them for several years. The railway authorities scoffed at the idea, and it was by the expenditure of very large sums that the Swedish merchant constructed for his own use the first tank-wagons. Scorn was immediately changed to enthusiasm, and to-day thousands of these wagons circulate on the railways of Caucasia and Griazi-Tsaritsine.

— The following appointments have been made at the Michgan Mining School: Dr. George A. König, late of the University of Pennsylvania, professor of chemistry; Edgar Kidwell, professor of mechanical and electrical engineering; Fred F. Sharpless, professor of metallurgy; Fred W. Denton, professor of civil and mining engineering. All these except Professor König have been connected with this school for several years as instructors, and have earned their promotion. Dr. Horace B. Patton has been appointed instructor in mineralogy and petrography; Dr. Alfred C. Lane, instructor in petrography and geology. These two have been connected both with the State Survey and with the Mining School for several years. Mr. Carroll L. Hoyt, a graduate of Cornell University in the mechanical engineering department, has been appointed instructor in drawing and mechanical engineering.

— A cuneiform tablet has been found at Tel Hesy, the ancient Lachish, by Mr. J. F. Bliss, who is excavating for the Palestine Exploration Fund. According to Professor A. H. Sayce of Oxford it contains the name of the same officer who is mentioned on tablets from Lachish, found some years since at El Amarna in Egypt.

— Sir John Lubbock will shortly issue, through the Messrs. Macmillan & Co., a work entitled "The Beauties of Nature and the Wonders of the World," uniform with his "Pleasures of Life."

— Messrs. Macmillan & Co. have in press, to be issued very shortly under American copyright, a long-expected "History of Early English Literature," by Rev. Stopford A. Brooke.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE AMERICAN ASSOCIATION AT ROCHESTER.

BY D. S. MARTIN.

THE recent meeting of the American Association for the Advancement of Science was in all respects a pleasant and successful one. The beauty of the city of Rochester, the absolutely perfect weather that lasted through the entire session, and the careful and systematic arrangements of the local committee all combined to favor the attending members. The number present was in all 455, larger than at any meeting in the past ten years, save the exceptional ones at Philadelphia, New York, and Washington, and ranking seventh in the entire series of forty-one meetings.

The sessions were held in the University of Rochester, whose handsome and commodious buildings are surrounded by a large and very beautiful campus. This latter was a constant source of enjoyment, like the university grounds at Toronto in 1889, where the midday interval could be passed under noble trees and on velvety grass, with the lake breezes to refresh the air. No pleasanter "environment" has ever been enjoyed than at Rochester; while the fine collections of the university in Sibley Hall and the proximity of the celebrated "Ward's Natural History Establishment" and the Warner Observatory gave added scientific interest.

A large number of local geological trips were made to points of interest in the neighborhood. Among these may be mentioned the great gorge of the Genesee and the Lower Falls, where the Clinton and Niagara rocks are so grandly exposed in section; the glacial deposits of the Pinnacle Hills, south of the city, which present some problematical features; and last, but not least, the rock-salt mines at Leroy and Livonia, some twenty to thirty miles southward of Rochester, where the great deposits of solid salt are reached at 1,010 and 1,369 feet of depth, respectively, and immense works are in process of construction. The age of these beds, as is well known, is salina, or perhaps more strictly waterlime.

The regular Saturday excursions arranged by the local committee had also much of a geological character, some going to Niagara, others to the Portage Gorge of the upper Genesee, and others to Stony Brook Glen, all of these being magnificent examples of stream-erosion.

Another matter of local interest was the opening to the members, by courtesy of the family, of the mansion and library of the late Hon. Lewis H. Morgan, president of the association in 1880 and eminent as a writer and student in archæology and ethnology. The visit to his library and collection was an occasion of gratification to many.

To specify or enlarge upon particular papers among the many and valuable ones presented, would be difficult and perhaps invidious. It is, however, but fair to say that especial interest, in sections E (geology) and H (anthropology), was developed in the active discussions that arose regarding two subjects — that of Comparative Geological Chronology as presented by Professor W. J. McGee, and Aboriginal Quarries of Flakable Stone by Mr. W. H. Holmes — both of Washington. Professor McGee's general doctrine is that, using erosion as a measure of time, it is possible to fix somewhat definitely the relative lengths of certain recent geological epochs, and then (as generally admitted on the basis of sedimentation, as by Dana and others) of the older and greater periods. Then, by fixing a date in years for the last glacial epoch, it becomes possible to estimate somewhat the duration of geological time. This last date, based partly on Croll's astronomical theory and partly on various strictly geological data, he would place at about 7,100 years ago. Using this as a unit of estimate, the relative time indicated by erosion, etc., to the "Columbian" deposits, is to this date as 30 ± to 1, giving about 200,000 years to the Columbian (early Quaternary); while the same process will require some fifty times as much, or 10,000,000 years, to the "Lafayette," late Tertiary. It is easy to see from these figures, when compared with the time-ratios for the geological ages as given, e. g., by Dana, how stupendous a time is demanded by Professor McGee's view, and how extreme is the difference between the geological requirements on the one hand and the duration allowed by the physicists and astronomers on the other. The discussion that arose was naturally active, and the subject is one likely to be prominent for some time to come.

Mr. Holmes has been investigating aboriginal quarries extensively, and presents the view that immense quantities of merely unfinished and rejected material at these points exhibit all the characters of so-called "palæolithic" work. He therefore questions strongly the palæolithic age of much that has been so regarded, certainly in this country. The discussion of this and other papers in the section showed a strong tendency to demand more proof, and that strictly stratigraphical, than has often been given in describing "palæolithic" implements and drawing inferences therefrom. Those who accompanied Mr. Holmes a year ago to his aboriginal quarry in the Potomac gravels at Piney Branch, near Washington, will remember that visit with increased interest in view of this important discussion.

Much else might well be mentioned, but space forbids. As a whole, it may be said that few meetings of the association have been more agreeable or more profitable than the one just closed at Rochester.

The decision to hold the next session at Madison, Wis., rather than at Chicago, is generally approved. The place is near enough to give the members opportunity to visit the World's Fair before or after the association meeting, and far enough away to escape the crowd and the distraction; while the provision made for a permanent headquarters for each section of the association during the entire period of the Fair, in rooms set apart for that purpose, is a most happy and desirable arrangement for the comfort and convenience of members visiting Chicago.

AMERICAN BOTANISTS AND NOMENCLATURE.

BY JOHN M. COULTER, PRESIDENT OF INDIANA UNIVERSITY.

THE Rochester meeting of the American Association was a notable one for American botanists. They had so burdened section F with papers in the years that are past that nothing was left but to organize them into a separate section, under the letter left vacant by the deceased Section of Microscopy. This calls for congratulation as testifying to the growing numbers and activity of botanists. Among botanists, however, the meeting was still more notable from the remarkable merging of all differences of opinion into an agreement concerning nomenclature.

This subject has not only brought botanists into conflict with each other, but into disrepute with fellow-scientists. Force seemed to be wasted in upholding varying personal opinions. So far as American botany was concerned, there seemed to be two hostile camps with

reference to nomenclature. How much of genuine good-feeling and exchange of courtesies existed under cover of this public hostility is known only to the botanists themselves. Every one desired a stable nomenclature, but the conservatives held so doggedly to the old, and the radicals ran so persistently to the new, that the result was chaos. It was speedily found that "good usage," which was founded upon individual opinion, could never bring stability in face of the fact that scores of botanists felt equally competent to stand for "good usage."

The culminatian of all these upheavals came in the famous book of Otto Kuntze, which looked like the end of all things to conservatives, and even made the radicals stand aghast. Kuntze wrought better than he knew, and has undoubtedly been largely instrumental in inducing a common movement among European and American botanists to attempt to secure some basis of agreement. His book will probable stand as a good example of what-not-to-do in matters of nomenclature. The International Congress of Botanists at Genoa (Sept. 4–11) was a favorable opportunity for presenting the matter, and hence the almost simultaneous appearance of papers from Berlin and New York and Washington for signatures.

At the meeting of the American Association at Rochester (Aug. 17-24) an unusually large number of botanists who deal with nomenclature were present, and they had with them (by letter) the opinions of nearly all who were absent. Not only was the representation very large, but the willingness to concede for the sake of agreement was remarkable, no such fraternal feeling being anticipated by the most sanguine. The discussions were full, free, and informal; every shade of opinion being presented and carefully considered. The principles that were finally adopted were not numerous, and additions will undoubtedly be necessary, but they were adopted with wonderful unanimity, and must commend themselves to anyone who studies them and who understands the forces that were at work in formulating them. Probably not a single individual opinion is fully expressed by these principles, but that resultant of opinions, which must be a far more influential thing.

The selection of 1753, the date of the first edition of Linnæus's "Species Plantarum," as the common point of departure for genera and species, seemed to be conceded almost without debate. This is no place to discuss the many very important considerations which urge the selection of this date; but it will certainly bring a feeling of stability in generic names that no other selection could have brought. It at once remands to silence all that region of uncertainty which necessarily lies beyond the time when species definitely stood as representing genera.

The fixity of the specific name has long been recognized as a working principle, and the only objection has been to making it an *ex post facto* law. But this would at once make two points of departure, and the changes are not so numerous after all.

The homonym section is also a wise one, as chiefly becomes apparent to those who have been compelled to reinstate an old group and so turn adrift and nameless some other group that may hold no relation to it.

It is probable that the section defining what is meant by the publication of a species will be the only one that will meet with criticism. To most of the botanists at Rochester, however, the definition strongly commended itself. The criticism will not be directed at what the definition contains, but at the fact that it omits the distribution of named specimens. This omission, however, can only touch chiefly comparatively recent distributions, for the names of the older classical ones have surely long since been protected by some form of publication which comes under the provisions of the section. The mixture of material under a single number in large distributions is not only well known, but probably to be expected, especially among plants in which the characters are microscopic. Herbarium names are also a great bar to the study of systematic botany, now that it has become a democratic thing, and a provision which compels all specific characterization to be widely accessible is a reasonable one.

It is to be expected that all American botanists will gladly use these principles, as it will remove a feeling of uneasiness in their work, a feeling which has sometimes compelled some of them to make sure of their species by mentioning the names they would bear under the different systems of nomenclature.

Names are things of secondary importance, and the long discussion of non essentials has seemed wearisome to many, but disputes are usually about non-essentials, are always wasteful of energy, and should always be adjusted.

CURRENT NOTES ON ANTHROPOLOGY.—XIV.

[Edited by D. G. Brinton, M.D., LL.D.]

The Selection of Comparative Vacabularies.

THE student whose investigations lead him to the comparison of languages and dialects is constantly impeded by the absence of any uniform schedule of words employed by travellers in securing specimens of them. This is one of the many points on which it would be most desirable that some international agreement could be reached.

The colonial department of the German government has recently published a schedule of about 800 words, which will be adopted by its officers and explorers. The list has been prepared by the eminent linguist, Professor Georg von der Gabelentz, and is published by Mitler & Son, Berlin, under the title "Handbuch zur Aufnahme Fremder Sprachen." It is prefaced by a series of practical observations and directions which will prove of much utility to the collector.

Our government has also an official schedule of words published through the Smithsonian Institution. It is a monument of colossal misconception of purpose and theory-hunting. The terms for kinship alone number 1476, and contain such as the following: "My mother's elder sister's daughter's daughter's daughter's husband!" Instead of being a convenient octavo, which one can slip in his pocket, as is the German, it is a bulky quarto of 250 pages, much of it taken up with quite useless matter. I venture the assertion with confidence that no collector has ever filled up its blanks.

Primitive Man in South America.

The doubts expressed in these "Notes" as to the age of some of the recent discoveries of anthropoid remains in South America (see *Science*, March 11) have been echoed with force by M. E. Trouessart in an article in *L'Anthropologie* for June. The hypothesis of a miocene man in the area of the Argentine Republic or Patagonia, advanced by Ameghino and others, has received a rude shock through the researches of Professor C. Steinmann of Freiburg. According to him, the Pampean formation corresponds to the Loess of North America, and is inter-glacial in date, and not pliocene, as Doering and Ameghino teach; and their alleged miocene is merely a part of the great deposit of the Austral glaciation. This he believes occurred at the same time as the ice age of the northern continent.

This opinion seems to be borne out by a comparison of the fauna of the oligocene of Patagonia with that of the alleged miocene of La Plata. The differences are quite too great for them to belong so near together. Twenty per cent of the Pampean forms are still living species in the same locality, which would be enough to cast grave doubts on its high antiquity. Here, therefore, as in so many other spots on the American continent, the vast antiquity of the remains of man is materially diminished by closer scrutiny.

Race and Culture.

A recent pamphlet by Professor Frank W. Blackmar, of the University of Kansas, on Indian education, brings up the general subject of the attitude of the lower races toward the culture of the highest. This sociological study, carefully prepared from authentic statistics, substantially acknowledges that while in individual instances there is no intellectual inferiority in the Red Race, its members are unable to face the light of civilization and live. Even when educated they must be protected, especially against their own people, but also against the whites. His final words are:—

"The Indian must be drilled, trained, and placed in an occupation which offers protection on the one hand and restraint on

the other. Otherwise he will not be able to compete with the white race in the economic struggle for land or the political struggle for power."

This is a sad conclusion, but it is that which is supported by the history of both the Red and the Black races, and is that which is illustrated by the histories of so many of the Polynesian islands, where the circumstances were most favorable to the development of the best relations between the natives and the Europeans. The psychic traits of races are as unalterable as the shade of their hair, and inevitably for them define the future of their stock and limit its possibilities.

The Land Fu-Sang.

Now that the discussion of the various discoveries of America is in order, that which is referred to in Chinese annals as far back as the seventh century, in connection with the name Fu-Sang, should receive attention. It was first brought to the notice of scholars in 1761 by the French orientalist, De Guignes, and of course created some sensation. Various writers since then have warmly espoused his views, among whom may be mentioned in our own country Charles G. Leland and E. P. Vining, both of whom have issued volumes in proof of De Guignes's identification.

The *coup de grace* seems to have been dealt the theory by Gustave Schlegel in his book published in Leyden this year entitled "Fou-Sang Kouo; le Pays de Fou-Sang." He is a Chinese scholar of acknowledged competence, and takes up the story as recited in the original, with as many side-lights as he can bring to bear upon it.

The result of his researches is to knock every pin from under the notion that any part of America could have been intended in the description of Fu-Sang. As far as any real land can be discerned through the fog of exaggeration and fable which encircles the whole account, it is that of the island Krafto or Saghalien, and the people described resembled the Ainos more than any others. A variety of arguments are adduced to show that Mexico is out of all question; and therefore those fanciful archæologists who have been ready to find Buddhistic elements in American religions will have to look for them elsewhere than in the legend of Fu-Sang.

Another Failure in Ethnic Osteology.

The trenchant criticisms of Professor Sergi of Rome have already been referred to in these notes. He has recently published another of these in which he attacks and apparently demolishes the favorite theories of Professor Kollmann of Basel, in relation to the analogy existing between the face and its members. The latter has long maintained that there is a constant correlation between the elements of the face of such a nature that to long faces correspond high orbits, narrow nasal apertures, and elongated palatine vaults; and to wide faces the converse of these characters; and that the types of races expressed in head-forms will be a composite of the cephalic and facial indices.

Professor Sergi arrives at quite a different conclusion. He points out from various series of skulls that in the purest types the craniological criteria vary very widely. In every race individual examples present the utmost diversity. As to any fixed correlation between the shape of the face and the facial indices, which is the *crux* of Kollmann's argument, it is a pure chimera. He presents a series of measurements, tabulated from African and American crania, which leave no doubt as to the accuracy of his assertions; and Dr. Colignon, who reviews his work for *L'Anthropologie*, accepts its conclusions as incontrovertible. This is another serious blow to that department of physical anthropology which has set up a few anotomical features as more important than those of language and mind, as criteria of peoples.

WE are informed that in view of the general interest awakened in the cholera, Dr. Klein's well-known little book on "The Bacteria in Asiatic Cholera," published by Macmillan, has been reduced in price to one dollar. Dr. Klein is lecturer at St. Bartholomew's Hospital, London.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

A Pre-Aino Race in Japan.

IN the Report of the National Museum for 1890, just issued, are two papers by Romyn Hitchcock, entitled respectively, "The Ancient Pit-Dwellers of Yezo" and "The Ainos of Yezo, Japan." In these papers he advances the idea, which he evidently thinks is new, that there was a race of people in Japan previous to the Ainos, and these people he identifies with the Pit-Dwellers of Yezo. He says, "it has been supposed that the shell-mounds were left by the Ainos. This is the opinion of Professor John Milne." Mr. Hitchcock further says, "It has recently been shown by the researches of Milne, Morse, Chamberlain, and others that Japan proper was once inhabited by a race of people different from the present Japanese, and from the comparison of the remains found in shell-heaps and kitchen-middens in many parts of Japan, even as far south as Kiushiu, with similar remains found in Yezo, it is thought that the Ainos once inhabited Japan." It is hardly necessary to inform Mr. Hitchcock that the writers above mentioned did not require the evidences of shell-heaps to convince them that the Ainos inhabited Japan, as historical records in that country fully establish the fact. I have always maintained, however, and in one case with an acrimony which I now regret, that all the evidences point to the existence of a race occupying Japan previous to the Ainos, citing these very shell-heaps as proof. I am not concerned with the fact that he has overlooked my views published at different times on the subject, but I do object most emphatically to being represented by Mr. Hitchcock as holding views directly the reverse of what I have repeatedly urged; and as the point of a Pre-Aino race in Japan, if established, is of some value, I do not intend to relinquish it unless other claims to priority can be shown. While Mr. Hitchcock has not taken the trouble to look up my papers on the subject, he cannot plead ignorance of my views, as he has made most ample use of a memoir by Mr. Basil Hall Chamberlain, published by the University of Tokio, and should have seen the following statements in that publication (p. 44). Mr. Chamberlain says: "Two theories may be held with regard to the former presence of the Ainos in Japan. One is that they have occupied the whole country before the arrival of the Japanese. This theory has been advocated by Professor Milne. . . . The arguments used by Professor Milne are chiefly derived from archæological finds. . . . To his arguments, which should be consulted at some length, . . . it has been objected by Professor Morse . . . that there is no positive proof that the remains attributed by him to the Ainos may not have been left by some still older race." There is, therefore, no excuse for this oversight or blunder on the part of Mr. Hitchcock.

Fifteen years ago I sent from Japan a communication to *Nature* of London, entitled "Traces of Early Man in Japan." In this I said: "The examination of a genuine kjoekkenmoedding, or shell-heap, enables me to give positive evidences regarding a prehistoric race who occupied this island." And when I designated this race as pre-historic, I supposed every one familiar with Japanese history was aware of the fact that the Ainos had preceded the Japanese in Japan, as the Indians had preceded the English in New England. Hardly a popular book on Japan had failed to allude to the fact, quoting early records of the Japanese in proof of it. Over thirteen years ago I sent an article from Japan to the *Popular Science Monthly*, entitled "Traces of an Early Race in Japan." This was published in the January number, 1879, and contained numerous engravings. In this paper I said: "With every reason for believing that the Japanese came from the south, displacing the Ainos, who came from the north, the question next arises as to the original occupants of the island. Did the northern people encounter resistance from a primitive race of savages, or were they greeted only by the chattering of relatives still more remote, whose descendants yet clamber about

the forest-trees to-day? The records are silent upon these points. A discovery that I made in the vicinity of Tokio last year leads me to believe that possibly the traces of a race of men previous to the Aino occupation have been found." Again I say: "The next question arises as to whether the deposits are Aino or pre-Aino. The race who left these remains were pot-makers *par excellence*. It is generally admitted by ethnologists that the art of pottery once gained is never lost. It is a fact, however, that neither the Esquimaux, Aleutians, Kamtchadales, nor the Ainos are essentialy earthen pot makers." And, again, having shown incontestible proofs of the evidences of cannibalism in these deposits, I ask, "Were the Ainos cannibals? Repeated inquiries among eminent Japanese scholars and archæologists, like Mr. Kanda, Mr. Ninagawa, and others, as to this question, are always answered in the same way. Not only were they not cannibals, but they are reported as being so mild and gentle that murder was never known to have occurred. So monstrous a habit would certainly have been known and recorded, particularly in the painstaking annals of early historians."

In the Proceedings of the American Association for the Advancement of Science for 1878 occurs in the list of papers read by title the following one of mine, entitled "Evidences of Cannibalism in a Nation before the Ainos in Japan." A foot note states that this paper was published in the Tokio *Times*

In the year 1879 the University of Tokio published my memoir on the "Shell Mounds of Omori," illustrating the various forms of pottery, bone implements, etc., by seventeen folded plates. While this memoir is devoted exclusively to a minute description of the Omori deposits as a basis of comparison with material that I had on hand for the description of other shell-heaps, yet I urged the evidence of the deposits not having been made by Ainos, but by a race anterior to the Ainos, and cited especially the evidences of cannibalism as bearing on this point.

Twelve years ago I had occasion to criticise and controvert (*American Naturalist*, September, 1880), in the most emphatic manner Professor Milne's views as published in the Transactions of the Asiatic Society of Japan. At the same time I also showed, as I believed, the fallacy of the views of Henry von Siebold on this question. Thus in various publications in 1877, 1878, 1879, and 1880 I have urged the existence of a pre-Aino race in Japan.

Had Mr. Hitchcock taken the trouble to give proper credit to others who had worked in this field, he would have found additional support to the position he takes; as it is, his paper is marred by misapprehension and by the injustice of these omissions.

EDWARD S. MORSE.

Salem, Mass., Aug. 30.

On the Fundamental Hypotheses of Abstract Dynamics; From Another Point of View.

THERE is at present very little agreement among physicists or philosophers as to the nature of the hypotheses or laws upon which dynamics is based. On Aug. 5 Professor MacGregor expounded one view of the matter in these columns; but as I cannot but think his view contains some logical imperfections, I wish to lay before your readers a different view with which to compare it. For this is not a question to be settled by authority; the arguments on either side are after all simple enough, and, having studied them, any man of average attainments is capable of weighing them and forming his own opinion.

The principles of abstract (subjective) geometry may be deduced from definitions of the terms "Position" and "Direction,"[1] together with certain axioms asserting the conceivability of geometrical figures and constructions. Even without these axioms a symbolic geometry might be deduced, whose conclusions, however, would be mere truisms, or verbal assertions, till they were given a meaning by the axioms. To proceed to the objective geometry of material space, we require in addition certain inductions; which, however, are so complete that no practical doubt remains as to their validity.

[1] See my "Foundations of Geometry," Deighton, Bell, & Co., Cambridge, Eng., 1891.

In the same way we may treat kinematics from three different points of view. Symbolically, it is sufficient to define Time implicitly by the assertion, "The positions of points are all continuous single-valued functions of the Time." This definition may be given a subjective meaning by the axiom, "Particles are conceivable in Time," and an objective meaning by an induction proving that "material particles exist only in Time," i.e., their positions are continuous single-valued functions of a certain variable, which we may call Time.

To proceed to kinetics symbolically, we require definitions of Mass and Force. The only connotation symbolically required for the former term is "Mass is *not* a function of Space or Time." The latter term may be defined implicitly by assertions equivalent to Newton's laws of motion, which may be stated thus: —

1. The resultant force on any particle in any direction, referred to a given set of axes, is the product of the measures of its mass and its acceleration in that direction.

2. All forces go in pairs between pairs of particles, equal forces in opposite directions acting on the particles respectively in the line joining them. (Such a pair of forces may be spoken of as a stress.)

It is evident from 1, since mass is not a function of space or time, that forces, like accelerations, are vectors, and may be compounded by the parallelogramic law. Paragraph 1, however, only speaks of resultant forces, and the actual, or acting, forces on any particle would remain entirely arbitrary but for paragraph 2, which must be read in conjunction with 1. Professor MacGregor asserts that paragraph 2 is not consistent (i.e., *might* be inconsistent) with 1. So far from this being the case, I propose to show that it still leaves the term Force to some extent arbitrary. The stresses between particles are not completely determined, even with reference to a given set of axes; and, moreover, both Force and Stress are relative to the axes chosen.

In geometry and kinematics both position and direction are relative terms. To determine a position we require to know its distance and direction from a given position. To know its direction we require to know the inclination of that direction to two given (independent) directions, and, in addition, which side it is of the plane determined by them.

Suppose, then, we have a set of particles numbered from 1 to n. Choose the first particle as origin of a system of rectangular coordinates; the direction 12 as that of the axis of x; the direction at right-angles to this in the plane 123, and on that side of the line 12 on which the particle 3 lies, as that of the axis y; and the direction perpendicular to the plane 123, on that side of it on which the particle 4 lies, as that of the axis z. Thus we have determined a set of axes completely, and in doing so we have made the six arbitrary assumptions: —

$$\left.\begin{array}{lll} x_1 = 0 & y_1 = 0 & z_1 = 0 \\ & y_2 = 0 & z_2 = 0 \\ & & z_3 = 0 \end{array}\right\} .$$

Now let F_{rs} be the stress between the particles r and s, being positive if they attract, negative if they repel one another. Then considering forces acting on particle 1 we have the equations —

$$F_{12}\,\frac{x_1 - x_2}{r_{12}} + F_{13}\,\frac{x_1 - x_3}{r_{13}} + \cdots\cdots = -m\,\ddot{x}_1,$$

and two similar equations with y and z (r_{12} being the distance between the particles). Thus in all we have $3n$ equations between $\frac{n(n-1)}{2}$ quantities F_{12}, F_{13}, etc. But these equations may not all be independent. As, however, they contain $(3n - 6)$ independent variables, x_2, x_3, y_3, etc. (the other six having been arbitrarily equated to zero), there will in general be $(3n - 6)$ of them independent. If they only just sufficed to determine the quantities F_{12}, F_{13}, etc., we should have

$$\frac{n(n-1)}{2} = 3n - 6.$$

Whence $n = 3$ or 4. Therefore, if n is greater than 4 (which, of course, it is), the equations must be insufficient to determine the quantities; that is, the stresses remain to some extent arbitrary;

hence the two assertions about Force are arbitrary and may be laid down as a (partial) definition of that term.

From this definition all the theorems of dynamics may be deduced, as from Newton's laws of motion. The theorems of statics may also be deduced, the only difficulty being the principle of virtual work. This difficulty, however, disappears as soon as the term "geometrical conditions" is properly defined.

We have then a symbolic dynamics. To give it a subjective meaning we have to conceive a real denotation for its terms. It is not, however, necessary to give a real denotation to Force if we can do so to Mass, for we may still regard Force as merely a name for the product of mass by acceleration, or (which is the same thing) as the time-flux of momentum. To give the theory an objective application it is necessary to show that what we call material particles not only occupy positions which are continuous one-valued functions of what we call Time, but also possess a certain characteristic which is not a function of space or time, and which may be called Mass. Then, whether we attach any denotative meaning to Force or not, we can discuss the forces or stresses that must be postulated between various particles of matter. The magnitudes of these will in general depend on the axes we assume by which to determine positions, and also on the masses assigned to the various particles. The axes and masses are therefore assumed in such a way as to make the resulting system of stresses the simplest possible. For example, it is generally assumed that the stress between any two particles diminishes as the distance between them increases, and may be neglected if this distance is very great. Hence in astronomy the attractions of the fixed stars on the planets may generally be neglected, and we may discuss the solar system alone. It is further shown that the system of stresses between the sun and planets is simplest when a certain plane is taken as "the invariable plane." But we do not really *know* that the stresses thus deduced are the actual ones, or indeed that there is any actual phenomenon corresponding to what we call stress at all. Any plane might be chosen as the "invariable" one, at the cost of having to postulate a more complicated system of stresses. We cannot determine fixed directions dynamically, any more than kinematically, except by making assumptions which are really arbitrary about the stresses between certain particles.

As Professor MacGregor points out, the law of the conservation of mechanical energy would flow from the assumption that stresses are functions of the distances between the particles on which they act. But this would not include the general law of conservation of energy until all energy was shown to be mechanical energy. And even then, on the above assumption, the term conservation of energy would be rather misleading; for the kinetic energy is not conserved unless the term potential energy is merely used as a cloak to hide our ignorance of kinetic energies which for the moment have passed beyond our ken. For example, a few years ago it might have been said that when we project a keeper away from an electro-magnet, the kinetic energy with which it starts becomes converted into potential by the time it stops, just as when we throw a stone into the air. But if, while the keeper is at a distance from the magnet, the current is switched off, that potential energy is *abolished!* The true view is, however, that there never was any potential energy at all, the energy of the flying keeper had its equivalent in an increase in the electric current round the magnet — a kinetic, not a potential, energy. And I have no doubt that some day science will show a similar explanation to hold with respect to gravitation and other actions at a distance. When that day comes the term "potential energy" may be banished to "the limbo of once useful things."

It will be seen, therefore, that I differ from Professor MacGregor chiefly in denying "the non-relative character of Force." Professor MacGregor says, "it is easy to show that if it [the third law of motion] hold for one point of reference, it cannot hold for another having an acceleration relative to the first." I should like to see his proof; but if he refers accelerations to a single point, I can well understand that he should arrive at results inconsistent with mine. For, as I have shown, the apparently absolute determinations of direction depend in reality on arbitrary assumptions as to stresses. Having made these arbitrary assumptions, it may well be impossible to further make arbitrarily the assumptions involved in the third law of motion.

I cannot quite follow his paragraph beginning "It may easily be proved that the stress between two particles is proportional to the product, by the sum of their masses into their relative acceleration." There seems to be some misprint; but how a single particle could in any case exert all the forces acting on a system of particles, I cannot understand, unless the words "equal and opposite" in the third law of motion are not held to imply that the forces act in the line joining the particles, which, moreover, is distinctly implied in the Professor's law of stress. In any case the difficulty referred to above comes in again, viz., that we cannot determine directions absolutely, or positions by reference to a single point.

In conclusion, I should like to point out that it seems inconvenient, even if Professor MacGregor's views be accepted on other points, to include in one law of stress, two statements resting on such very different evidence as that forces may be considered to be attractions or repulsions, and that their magnitudes depend solely on the distances between the particles on which they act. It would give a student a very false notion of the fundamental hypotheses of dynamics to teach him that he must accept or reject both these assertions together.

EDWARD T. DIXON.

Cambridge, Eng., Aug. 20.

The Fundamental Hypothesis of Abstract Dynamics.

PROFESSOR HOSKINS points out (*Science*, Aug. 26, p. 122) that for the conservation of energy the necessary and sufficient condition is that ΣPdr shall be a perfect differential of a function of the quantities r, P being the stress between any two particles of the system, and r their distance; and that the condition that each P shall be a function of the corresponding r only, which I suggested for adoption as a fourth law of motion, with a view to the deduction of the law of the conservation of energy (*Science*, Aug. 5, p. 74), while sufficient, is not necessary.

There are three reasons which influence me in selecting for the fourth law an hypothesis which is more than sufficient for the main purpose in making the selection, viz., (1) that it is capable of simple physical expression, (2) that it is already known to hold in the case of several natural forces, and (3) that the additional assumption involved in it, over and above that necessary for the deduction of the conservation of energy, is one which is, I think, invariably made in investigations on the laws of natural forces.

What the additional assumption is, is readily seen. In a system of two particles A and B, ΣPdr becomes Pdr; and in this case it is both necessary and sufficient for the conservation of energy that the single stress acting shall be a function of the distance AB only. If we add a third particle, C, to the system, conservation no longer requires that the stress between A and B shall be a function of the distance AB only, though it is secured if that condition is fulfilled. Thus the proposed law assumes, in addition to what is required for conservation, that the stress between A and B is not changed by the fact that other stresses have begun to act between A and C and between B and C. The proposed law therefore involves an assumption similar to that implied in Newton's second law. As Newton's law assumes that a force produces the same acceleration in a particle whether other forces act on it or not, so the proposed law assumes that the stress between two particles is the same whether or not there are other stresses acting between them and other particles.

That this additional assumption holds in the case of some natural forces has been abundantly verified, and in investigations into the laws of forces not yet determined, so far as my knowledge of such investigations goes, the same assumption is always made. This being so, we would seem to be warranted in adopting, tentatively of course, as a fourth law of motion an hypothesis in which this assumption is implied. The proposed law cannot be said to have received anything like the verification that Newton's laws have received. But of the many deductions which have been made from it, none have been contradicted, while many have been corroborated, by experience.

J. G. MACGREGOR.

Shubenacadie, N.S., Sept. 2.

The Nomenclature Question.

I AM glad to see this question brought up as it is by Professor Underwood in the number of *Science* for Aug. 26; for we should have a uniform nomenclature in all departments of natural history. That such is not the case now is apparent to every student who is working in any of its various branches. But I do not wish to discuss the subject in general, but to touch upon one or two points. As to the question of priority, there should be some definite rules by which this should be governed, as has already been said in other of our scientific periodicals, and it will not profit by any rehashing it here, further than to say that among entomologists it is generally understood that the mere proposal of a name for a genus without characterizing it does not hold against a later name accompanied by a description.

As to the act of a writer who takes a species already named and puts it into a new genus with his own name after it instead of the name of the original describer, that is an outrage that has not been tolerated among entomologists for some time. I can see no valid reason for retaining such a system of nomenclature in any department of natural history, merely that some reviser may gain a little cheap notoriety.

A word as to the initial letter of specific names. It seems to me that the name of a species is a proper name as much as the name of a genus; in other words, it is the name of a group of plants or animals, and, if such, is as much entitled to a capital initial as is the name of the genus. Many of our leading entomologists have adopted this view and begin all specific names with capitals; as, for instance, see Edwards's "Revised Catalogue of the Diurnal Lepidoptera of North America," 1884; Kirby's "Catalogue of Diurnal Lepidoptera," 1871 and 1877, etc. I believe it is the correct principle and follow it in all my work in natural history.

G. H. FRENCH.

Southern Illinois Normal, Aug. 30.

The Grand-Gulf Formation.

THIS has now become a clearly recognized division of the post-eocene geology of the Gulf States. No subdivisions of it have as yet been attempted in print, though more than three years have elapsed since the writer — then in the service of the U. S. Geological Survey — announced the first discovery of fossils on Pascagoula River and the two branches which form it, Leaf River and the Chickasawhay, near their junction. The exact locality of the largest deposit is Shell Bluff, just below Robert's Ferry and a few miles south-west of the post-office Vernal, in Greene County, Mississippi. It was then proposed to call it the Pascagoula formation, and to regard it as distinct from Dr. Hilgard's Grand-Gulf. Further developments and recent discoveries have confirmed me in this view. It was not at first accepted, because there is but the one witness, myself, and attempts to trace it westward and eastward failed to detect the same or similar fossiliferous beds on the Mississippi, on Pearl River, on the Alabama River, or on any of the smaller streams of these States. This kind of negative testimony would only go to restrict its extension, and not to overthrow the validity of the distinction if otherwise properly established.

Many facts, too numerous to be elaborated in this short paper, prove that the great Mississippi embayment had collateral branches in which the variations are too well defined to be disregarded. The Pascagoula embayment was one. And whilst the main body of the Grand-Gulf formation is of sand, sandy clays, and quartzites due to a fresh-water agency, in the Pascagoula formation it presents a marine aspect, where calcareous clays, more or less pure and with more or less distinct evidence of molluscan fossils, prevail. The boundaries of these two will not be attempted in this paper. Let us pass at once to some of the strongest and more recent proofs.

Of the shells discovered at Shell Bluff it may be said only one, the large oyster, could be clearly determined. The rest were in a condition so decayed and friable as to render their transportation in good condition impossible. But as I remember them, the oyster approached, yet differed from, the recent *O. Virginiana*, among other particulars, in its greater massiveness. Among the other shells too rotten to be moved was one strongly similar to a *Gnathodon*, though it may turn out to be a *Mactra*. Another, and the most numerous, was a small shell somewhat resembling in size and outline the *Donax* so common on our beaches, but with less umbonal development, and with the distinctly visible lines of growth resembling *Venus*. The difficulty in this case as well as the other is that the hinge could not be clearly made out.

Borings for artesian wells at Biloxi and other places on the Mississippi coast, and quite recently at Mobile, Ala., solve the difficulty.

The Biloxi borings, among other things, brought up, from a depth in the neighborhood of 700 feet, fragments of a large oyster, which might well belong to that of Pascagoula, and a very easily recognized *Gnathodon*.

The boring at Mobile, from about the same depth and just above the water-bearing sands, has yielded similar bits of oyster, and a small shell, evidently the same as that of Pascagoula, and sufficiently preserved to be determined. It is a *Venus*, or very nearly allied to that genus, and if not already found elsewhere and named, the name *V. Mobilensis* is proposed for it.

Not having room to go further into detail, I wish clearly to say that I find evidence sufficient to establish the existence of a formation of deep-bedded gray clays of partially marine genesis, lying upon the water-bearing sands of the upper strata of the Grand-Gulf formation; that I have traced this clay from Pearl River, Miss., to Conecuh River, Ala.; that it constitutes the cover rendering artesian wells possible, and that it was for these clays that the name Pascagoula was proposed.

LAURENCE C. JOHNSON.

Meridian, Miss., Aug. 1.

European Origin of the Aryans.

My attention has been called to Dr. Brinton's note in *Science* for June 20 as to the claim of Omalius d'Halloy to have preceded Latham in calling in question the theory of the Asiatic origin of the Aryans. In 1890, when in his lectures on "Races and Peoples," Dr. Brinton advanced the claim of d'Halloy, I carefully read over Halloy's articles, as cited by Dr. Brinton on p. 146 of his book, and I came to the conclusion that d'Halloy was not acquainted with the theory he is said to have controverted. The dates confirm this conclusion. The articles in question were published in the Bulletins of the Belgian Academy during the years 1839 to 1844, and were recapitulated in 1848. The theory of the migration of the Aryans from central Asia first found definite expression in an article by Pott, buried in a volume of Ersch and Grüber's Encyclopædia, which was published in 1840, but it attracted no attention till taken up by Lassen in 1847, and by Jacob Grimm in 1848. This was the theory against which Latham contended, whereas d'Halloy's very confused and misty arguments seem to refer, if they refer to anything, to the Caucasian theory broached by Blumenbach in 1781, with the modifications proposed by Adelung in his Mithridates, 1806–1816.

I think, therefore, we are still justified in asserting that Latham was the first to question the comparatively modern theory that the Aryan race originated in the highlands of central Asia, a theory of which d'Halloy does not seem to have heard, and consequently in the second edition of my "Origin of the Aryans," published in 1892, I did not think it necessary to modify my former statements as to Latham's priority.

ISAAC TAYLOR.

Settrington, York, England.

Acid Prevention of Cholera.

IN previous epidemics the value of sulphuric and sulphurous acids as preventives was demonstrated, and when Koch discovered his comma bacillus he also noted that its cultivation was possible only in alkaline media, and that acids destroyed it. In corroboration of these findings, Niemeyer, who wrote long before anything of this nature was known, records that the ileum, or lower small intestine, is the main seat of the pathological changes caused by cholera. This lower small intestine is the most alkaline and the farthest from the normally acid stomach. The large intestine, being acid, does not suffer.

In view of these discoveries it would be well to establish an acid condition of the system by ten or fifteen drops of sulphuric acid to the quart of water used as lemonade — the water previously boiled,— and observe if sour wines might not be better for those in the habit of drinking liquors, also as to whether gout and rheumatism, which are acid diatheses, conferred immunity.

S. V. CLEVENGER, M.D.

Chicago, Sept. 5.

Mars.

AT the present time, while theories and suggestions concerning the planet Mars are in order, it might be well to note that, on a study of Schiaparelli's chart of Mars, the systems of so-called canals resolve themselves, in many cases, into radiating groups of six, making hexagons, and giving the idea that the planet may be solidified into a mass with tendency to hexagonal crystallization, the "canals" being, for instance, fissures on the lines of the angles of crystallization. This would account for many of the peculiarities of their appearance, while in no way opposing the present existence of atmosphere, water, snow, ice, and vegetation on the planet.

C. W. KEMPTON.

Oro Blanco, Ariz., Aug. 25.

La Grippe.

THE name *La Grippe* as used to designate the influenza, which was epidemic over so large a part of the world during the past two or three years, seems to have had a curious origin. Dr. Grant, in an essay on the disease published in 1782, states that the French term *La Grippe* is derived from an insect of that name remarkably common in France during the previous spring, and which the people believed contaminated the atmosphere, and caused the disease. If this be true, what insect was it?

M. L. HOLBROOK.

New York, Aug. 29.

BOOK-REVIEWS.

A Journal of American Ethnology and Archæology. Editor, J. WALTER FEWKES. Vol. II. Boston, Houghton, Mifflin & Co. 1892.

THIS volume is issued as one of the publications of the Hemenway South-western Archæological Expedition, and embraces, I., A Few Summer Ceremonials at the Tusayan Pueblos, by J. Walter Fewkes; II., Natal Ceremonies of the Hopi Indians, by J. G. Owens; III., A Report on the Present Condition of a Ruin in Arizona Called Casa Grande, also by Dr. Fewkes.

Dr. Fewkes, the editor of the journal and the author of two of the contributions to this volume, has treated the subject of the Tusayan ceremonials with much greater success than were treated the Zuñi rites, to which he devoted much of the first volume.

The province of Tusayan, or so-called group of Moki Indian pueblos of north-eastern Arizona, owing to their remoteness from the demoralizing influence of the white-man's civilization, are among the most primitive of our aboriginal tribes, and Dr. Fewkes has made no mistake in abandoning the Zuñi field (to which he devoted his first field-season, and to which the attention of such workers as Mr. F. H. Cushing and Mrs. M. C. Stevenson had earlier been drawn) in order to apply all his energies to this interesting people. So far as ethnologic investigation has proved, the Tusayan group (excluding the Tewa village of Hano) is the only existing example of a nomadic people adopting a strictly pueblo life — for the Mokis, or Hopi, are a part of the great Shoshonean stock; cousins of the Utes, the Snakes, and the Comanches, and who, centuries ago, were disconnected from the main family and forced to these mesa fastnesses, where they erected communal structures of stone and mud, and cultivated corn, squashes, cotton, and other products in the sand-spread plains below.

Many of the ceremonials described by Dr. Fewkes in this volume have evidently been borrowed by the Tusayan from the

various Rio Grande tribes and from the Zuñis — those from the former doubtless through the Tewa who fled from the Rio Grande during the great Pueblo revolt against Spanish authority in 1680–96. Borrowed ceremonials, however, undergo great changes, as exemplified by the "ghost dance" or "Messiah craze" now so general among the tribes from the British possessions to the Mexican frontier; hence it is not improbable that many of the Tusayan dance-dramas, which originated, say, in Zuñi, are now recognizable only by the corrupted Zuñi names which they still retain.

A number of the similarities of the Zuñi and Tusayan summer ceremonials are shown by the author, the performers and their paraphernalia minutely described, and many interesting features brought to light. The paper is a valuable contribution to science. The time for original research among the Pueblo tribes is rapidly disappearing, and, happily, Dr. Fewkes is losing no time in placing before the scientific world the results of his observations.

The second paper — Natal Ceremonies of the Hopi Indians, by Mr. Owens — is a very pretty portrayal of the birth-rites of the Tusayan, or, as they call themselves, the Hopi Indians, and many interesting facts are made known. Mr. Owens, who was Dr. Fewkes's field assistant, records these ceremonials without attempting their probable interpretation — a wise precaution, since, without at least a fair knowledge of the native tongue, or an intimate acquaintance with the Indians themselves, the results might otherwise have been misleading.

Ever since the first establishment of missions by the Jesuit Father Kino, in southern Arizona, in the 17th century, the civilized world has been treated to descriptions of Casa Grande, a massive ruined adobe structure a short distance from the banks of the Rio Gila. Some of the authors aver that this noble old building was the birthplace of Montezuma, and on many of our maps of the latter half of the last century it is noted as the second stopping-place of that monarch on his way from Aztlan! Several authors agree in identifying it with Chichilticale, a ruin mentioned by Vasquez de Coronado in 1541; but this Bandelier denies on the ground that the course of that *conquistador* lay farther east. Dr. Fewkes has supplemented the information given by Bartlett, and later by Bandelier, Hinton, and others, with a description of the present appearance of Casa Grande, accompanied by a number of excellent illustrations and a ground-plan on which various measurements are given. A reference, on page 189, to what appears to be an accidental clogging up of an opening in one of the walls by debris fallen from above, should not stand uncorrected. The massive and symmetrical block of abobe referred to and figured in one of the cuts is a door "close," examples of which, but generally of stone, are frequently found in our south-western ruins, and which were formerly in use by the Zuñi Indians. Indeed, the Zuñi name for door is but a survival of the term, now obsolete, of course, for stone-close; i. e., when door were introduced, doubtless by the Spaniards, they were still closes to the Zuñi mind, and since their name for a close was, literally, "stone close," their name for a wooden door became "wooden stone-close," a name which is retained to this day. The block of adobe was a close, and was fashioned to fit the opening of the wall, thus forming a cumbersome but sure means of defense.

The volume is a model of typography, and it is illustrated generously and well.

F. WEBB HODGE.

Washington City.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19, 1891. No response to date.

N. D. C. HODGES,

874 BROADWAY, NEW YORK.

TO THE READERS OF SCIENC

PUBLISHER'S ANNOUNCEMENT.

Titles of Some Articles Published in *Science* since Jan. 1, 1892.

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Agriculture, Experimental, Status of.
Amenhotep, King, the tomb of.
Anatomy, The Teaching of, to Advanced Medical Students.
Anthropology, Current Notes on.
Architectural Exhibition in Brooklyn.
Arsenical Poisoning from Domestic Fabrics.
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Inventions in Foreign Countries, How to Protect.
Inventors and Manufacturers, the American Association of.
Iowa Academy of Sciences.
Jargon, The Chinook.
Jassidæ; Notes on Local.
Keller, Helen.
Klamath Nation, Linguistics.
Laboratory Training, Aims of.
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Lightning, The New Method of Protecting Buildings from.
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Maize Plant, Observations on the Growth and Chemical Composition of.
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Museums, The Support of.
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Polariscopes, Direct Reflecting.
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Wind-Storms and Trees.
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Zoology in the Public Schools of Washington, D. C.

Some of the Contributors to *Science* Since Jan. 1, 1892.

Aaron, Eugene M., Philadelphia, Pa.
Allen, Harrison, Philadelphia, Pa.
Baldwin, J. Mark, University of Toronto, Canada.
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Beal, W. J., Agricultural College, Mich.
Beals, A. H., Milledgeville, Ga.
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